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ABSTRACT

The failure to attain a true synthesis of six ethnographic studies of interracial education in desegregated schools is examined. Detailed individual descriptions of desegregation processes were yielded but the failure of summary attempts is attributed to the lack of a theory of social explanation appropriate to interpretive social science, and a lack of understanding of the relationship of social research to practice. A policy-relevant method of ethnographic synthesis considering the nature of explanation is presented. The essay summary and full report summary formats were aggregate result approaches placing aggregation above understanding. Context became the confounding variable in the search for common findings. A truly adequate explanation is much more involved and requires a comparative understanding by defining a "puzzle" of conjunctions of hypotheses for an explanation that "translates" the practices and conditions within the schools to each other, lessening the research contraints of policy. Both the hypotheses and the explicit comparison are maintained to guard against the threat of utilitarian culture on ethnographic syntheses. Utilitarian culture seeks to explain struggle, power and emotion as technical problems in prediction and control. (CM)

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Not Seeing the Forest for the Trees:

The Failure of Synthesis for the Desegregation Ethnographies

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In 1975, the National Institute of Education awarded six contracts to conduct ethnographic field studies of urgan desegregated schools. The decision to make the awards was controversial, but in the end Ray Rist was able to convince other NIE officials that such studies were appropriate to inform the research agenda of the newly formed Desegregation Studies Team. For NIE's purposes, studies that focused on the process of interracial schooling promised to generate new insights. Later, Rist (1979: p.7) was to write:

> Despite the pressing need to learn more of the political and social dynamics of school desegregation, of the interactions within multiracial student populations, and of how schools learn to cope with new discipline and community relations matters, the research has been extremely limited. The very large majority of studies have not been grounded in the analysis of the day-to-day working out of school desegregation.

The resulting studies did accomplish this objective. Almost three years later, detailed analyses of the schools were submitted to NIE, and NIE reviewers dubbed them successful studies but gloomy in terms of the prospects for school desegregation. The schools studied all had been initially chosen because they were reputed to be good examples of desegregated schools within the respective school districts. Yet the research teams found that even in these good schools, desegregation was not being smoothly implemented and had encountered significant resistance on many fronts. There was considerable difficulty in the schools in even defining the appropriate meaning of desegregation. Further, in absence of such agreement, "business as usual" (Sagar and Schofield, 1979) was the reaction of schools. As a result, resegregation of the schools and classrooms ensued; order became a heightened priority; and the individual student, student, and administrator were left to fend on

his/her own (Rosenbaum, 1979).

The ethnographies, of course, revealed much more than can be summarized here and interestingly raised for NIE and us, as the researchers, the problem of how to reduce the detailed site-specific findings into a set of results that could be communicated to policymakers. In the end, two rather different approaches were attempted under the guidance of Murray Wax. One approach was to summarize the similar lessons from all sites (Wax, 1979a) by condensing the five final reports submitted (one contract did not submit a final report in time to be included). This approach, however, was criticized in the same volume by LeCompte (1979) as not being able to credibly and convincingly support the conclusions due to a lack of detail and because it ignored the idiosyncratic differences which seemingly were also policy relevant.

The other approach was to let the research teams themselves agree on a set of salient issues and to conduct cross-site analyses based on the data from all the sites. This attempt did not include a set of commissioned critiques as did the other attempt but only a summary by Rosenbaum, an independent analyst (Wax, 1979b). (My above summary of the results is drawn from that attempt.)

Regardless of which summary attempt is involved, it seems that, in large part, LeCompte's critique applies to the cross-sites essays as well as to the final reports' summary. She, I think, reasonably argued:

> The Wax summary of five ethnographic studies of desegregated schools promised a great deal but does not deliver as much as it promises. Both Practitioners and Academics reading such a document will be looking for answers.--though of a different kind. Teachers, administrators, and politicians will be looking for guidelines and techniques that they can utilize toward the immediate solution of a pressing problem; Academics will be hopeful of an explanation of the complex phenomena under examination, or at least a conceptual framework, consistently applied, which might explain variation in the phenomena. Neither are provided, although some useful

> insights can be teased out of the material presented. While it is difficult to quarrel with the well-stated initial premise -- that ethnography is a particularly useful tool for studying processes such as those involved in desegregation of schools, and is a technique which provides insights garnered by no other means -- the brevity of the report has obviated the richness of data and explanatory detail which is the hallmark of good ethnography and permits its conclusions to be well-grounded. What remains are some rather trite and atheoretical explanations for the failure of schools really to desegregate -- such as the absence of effective leadership from the principals -- and an idiosyncratic view of the whole process of desegregation which ignores some of the more important structural aspects of the conflict inherent in such a situation. In short, the article under review earns plaudits for what it attempts to do and some serious criticisms for what it fails to do)LeCompte, 1979: 118).

As condeming as this critique is, I believe it is instructive in that it proposes that the summar attempt does not end up being either truly ethnographic or policy-relevant. The desegregation ethnographies and the various attempts to communicate their findings (Clement, Eisenhart, and Harding, 1978; Collins and Noblit, 1978; Ianni et al., 1978; Scherer and Slawski, 1978; Schofield and Sagar, 1978; Rist, 1979; Henderson, 1981) all were able to offer adequate analyses of each site. Yet how do we account for the failure at the attempts at ethnographic synthesis: I propose the failure to attain a true ethnographic synthesis for the desegregation ethnographies is common to all attempts at ethnographic synthesis. Essentially, the failure is attributable to our lack of understanding on two fronts. First, we simply lacked a theory of social explanation appropriate to an interpretive social science. Without this theory, we reverted to summations rather than explanations. Second, we lacked an understanding of the relationship of research to practice, and how social explanation and practice are related. Fortunately, I believe both of these problems can be overcome and in the remainder of this paper I will explain the

rudiments of a policy-relevant ethnographic synthesis. In general, it seems to me that we focused so concretely on the data that we ignored the nature of explanation, and ended up "not seeing the forest for the trees".

The Problem of Ethnographic Synthesis

The two summary attempts (Wax, 1979a, 1979b) were essentially similar in that they focused on attaining "general" conclusions:

> At a meeting in November, 1977, agreement was reached that the investigating teams should participate in a small venture toward achieving more general conclusions from the ethnographic specifics of the separate cases. Each team proposed or was assigned a particular topic or theme, with the notion that its members could secure relevant data concerning each of the other sites. Thus, instead of five final reports, each of which might have mentioned something about a topic such as the relationship of lower-class black students to the school, there would be a single essay integrating the findings about the alientation of such students from schools that were supposedly desegregated (Wax, 1979b: 1).

> The present work constitutes an attempt to summarize and integrate the major findings of those five final reports into a compass of about 30,000 words. The work was commissioned with the hope that the process of textual integration would serve to bring forward the common findings among the five investigators, while the shortened size would mean a wider audience than might be gained by any single report. Moreover, it was also hoped that the textual integration would lead to a de-emphasis of the faults and virtues of the particular sites while focusing attention on the common problems entailed in desegregating the schools (Wax, 1979a: v).

The summary attempts were expiermental in that two different approaches, the cross-site essays (Wax 1979b) and the final report's integration, were pursued. Yet the experiment did not vary how we were to summarize, only what we were to summarize: both aimed to isolate the common findings and de-emphasize the uniqueness of each site. Thus the experiment was to compare the essay summary format with the full report summary format, both based in seeking common findings. While both are not an unusual way to summarize findings, they entail an unstated theory of social explanation that focused on aggregate patterns of results and as such is akin to positivism although we did not understand that at the time.

One might ask what is wrong about this approach. There is little wrong except that the aggregation we engaged in: 1) avoided a full exploration of context, and 2) did not enable an explanatory synthesize. Since the publication of the summary attempts, Turner (1980) has provided us with a "theory of social explanation" that enables us to better understand what went wrong and what might be done about it. Turner's formulation is especially appropriate to ethnographic analyses in that it is based in Winch's thesis and builds upon the critiqued thesis to propose a theory of social explanation based in comparative understanding, thus rejecting the physical science analogy of positivistic social science.

The desegregation ethnographies' summary attempts seemingly belied the rudiments of an ethnographic approach in that it ignored "meaning on context" (Nishler, 1979). As Rosenbaum (1979b) concluded from the cross-site essays, desegregation did have many different meanings in the schools studied. Yet this example reveals jow context was treated. That is, context became the confounding variable in the search for common findings. The logic of the summary attempts essentially placed aggregation above understanding, and left us in the difficult situation of attempting to discount the effects of context. Only the Sullivan (1979b) essay escaped this trap. By concentrating on context and conducting a comparative analysis of the five sites, Sullivan was able to assess how context affected desegregation and vice versa. Unfortunately, Sullivan's aatempt was so powerful that it resulted in an editorial decision to delete contextual descriptions from the other essays, and possibly contributed to overall failure to attain synthesis.

Sullivan's summary attempt is instructive in another way. His comparative analysis, much like Turner's (1980) proposal, is the keystone to ethnographic synthesis. Not only does it maintain context as a salient

component of analysis, it also avoids the aggregation issue. That is, he was not tempted to make general conclusions: the aggregation of uniqueness was simply nonsensical.

The aggregation approach to ethnographic synthesis does more than ignore context, it stops short of explanation. Turner (1980) argues:

> Analysis of aggregate patterns can help set up puzzles, and differences in aggregate patterns may require explanations that cite differences in practices. But the question "Why the different practice?" is not touched by the analysis. (p. 97)

It is Turner's contention that a truly adequate explanation is much more involved than simple aggregation and interpretation of the resulting aggregate patterns. A truly adequate explanation requires a comparative understanding and is much like a translation:

> Earlier it was remarked that the relation between translation and explanation was sometimes intimate. We can now see that the logical relation between this sociological issue and the translation issue is more than intimate. The hypothesis to be tested is a conjunction of the two hypotheses, that is, the two hypotheses taken together. One cannot test the one hypothesis without assuming the adequacy of the other. But because one does not ever test the two types of hypotheses separately, this "assumption" is more reasonably regarded as part of the hypothesis itself. Thus each translation has a "sociological" component of practices that are assumed to be followed. The fact that we test translations, or at least intelligibly argue for and against them, means that the sociological component is tested as well. So we have a criterion for evaluating the validity of the sociological component in the same sense that we have criteria for evaluating the translation. The criterion is exactly the same, because it applies to the conjunction and not to the translation or the sociological component separately. There is a recognizable sense in which the sociological component is a comparative explanation. So here we are dealing with a sociological explanation that we assess as we assess a translation, and in this sense we are treating sociological explanation as translation (Turner, 1980: 60-1).

Turner's formulation essentially argues that an adequate explanation sets a comparative "puzzle" (the conjunction of two hypotheses as above) and assesses the evidence regarding the puzzle, and rejects or reformulates the puzzle:

> In defining the puzzle, we proceed as though we hypothesized that where we would follow such and such a rule, the members of another social group or persons in another social context would do the same. This was called the same-practices hypothesis. The puzzle is set by identifying the breakdown in the hypothesis. The explanations that constitute the solutions to these puzzles, it was suggested, are kin to another, familiar, kind of explanation: the explanation of a game "by describing one as a variation of another -- by describing them and emphasizing their differences and analogies.": The different practice in a social group or social context that raises the puzzle is explained in the way that a different rule of a game is explained (Turner, 1980: 97).

This form of explanation does not require a prior knowledge or theory to formulate and solve an adequate puzzle. In fact, Turner responds to a question about the necessity for a general framework:

> What is logically peculiar about the .... question is that it seems to rest on the idea that "what is important? is something that can be decided in advance of explanation or apart from it. It is illicit to prejudge the question of which facts about society are truly "fundamental".... Assessments of what is fundamental, if they are ever intelligible as factual claims, may be based on factually valid explanation, and not vice versa (Turner, 1980: 77).

The aggregation approach to ethnographic synthesis that we employed in desegregation ethnographies not only was context-stripping, it impeded explanation and thus negated a true ethnographic synthesis. The aggregation across context procedure only defined an set puzzles and because of the focus on commonalities probably resulted in inadequate definitions of the puzzles. Better puzzle definition would have resulted from a focus on the conjunction of two same practice hypotheses. This would have allowed

context as part of the explanation and would have required an explanation that "translates" the practices and conditions in one school into the practices and conditions of the other schools. In short, LeCompte's (1979) critique of the final reports' summary (but applicable to the cross-site summaries except possibly Sullivan's attempt) is apt. We did fail to provide the explanation that Academics might have wished, and, as is common in all research, the failure is attributable to methodology employed. We simply did not have the advantage of Turner's (yet-to-be-published) theory of social explanation, for LeCompte's solution to focus on similarities and differences is also an aggregation attempt at synthesis. Thus while her critique is apt, her solution is lacking.

The Practice Puzzle

The second component of LeCompte's critique was that the summary attempts failed to inform practice as it had also failed to inform academics. She writes:

> Part of the summary document focuses on causes of failure internal to the school establishment itself. So doing reinforces the belief, common among educators, that the solution to social problems lie in some magical manipulation of the clients, structure, or employees of the school, rather than in some major rearrangement of social structure relationships in society (LeCompte, 1979: 122).

This critique of the final reports' summary certainly has its points. First, it points to the problem of levels of analysis. The problem that an organization or program experiences may be a larger problem of social institutions and thus simply not amenable to a solution on the organization or program level. Second, it points to the falacious assumption that all problems are amenable to rational administrative solutions. As Mannheim (1936: 115-116)

writes:

> There is no question that we do have some knowledge concerning that part of social life in which everything and life itself has already been rationalized and ordered. Here the conflict between theory and practice does not become an issue because, as a matter of fact, the mere treatment of an individual case by subjecting it to a generally existing law can hardly be designated as political practice. Rationalized as our life may seem to have become, all the rationalizations that have taken place so far are merely partial since the most important realms of our social life are even now anchored in the irrational. Our economic life, although extensively rationalized on the technical side, and in some limited connections calculable, does not, as a whole constitute a planned economy. In spite of all tendencies toward trustification and organization, free competition still plays a decisive role. Our social structure is built along class lines, which means that not objective tests but irrational forces of social competition and struggle decide the place and function of the individual in society. Dominance in national and international life is achieved through struggle, in itself irrational, in which chance plays an important part. These irrational forces in society form that sphere of social life which is unorganized and unrationalized, and in which conduct and politics become necessary. The two main sources of irrationalism in the social structure (uncontrolled competition and domination by force) constitute the realm of social life which is still unorganized and where politics becomes necessary. Around these two centres there accumulate those other more profound irrational elements, which we call emotions (pp. 115-116).

Some problems are amenable to rational solutions, while others require political ones. In the case of desegregation, LeCompte's critique is again apt on both points. The desegregation issue is not essentially a school problem although that in no way justifies segregated schools. Further, desegregation, regardless of the proposals of the planned change approach, is not as amenable to rational solutions as many would think. Much of what the individual ethnographies documented was the politics of race and class in the schools and classrooms.

However, LeCompte's critique fails again to point to an alternative that

would have enabled the ethnographic synthesis to inform policy. In large part, it could be argued that the mistake is not defining the essential puzzles of practice. That is to say, if explanation is translation, then research to inform practice must define, set and assess some practice puzzles about research and practice. The puzzle would ask what is different about the research and practice in question and offer a comparative explanation.

While certainly each issue on which research hopes to inform practice would require a somewhat unique puzzle, it is possible to detail the form of the puzzle, if not the exact content. Shackle (1966: 767) gives us a direction:

> In everyday language and in that of the policy sciences, decision includes two quite contrasted meanings. Two contrasting psychic activities, two attitudes to life and two different types of mind are involved. There are truth-seekers and truth-makers. On the one hand, the pure scientist deems himself to be typically faced with a problem which has one right answer. His business is in the map-maker's language, to get a fix on that problem, to take bearings from opposite ends of a base-line and plot them to converge upon the solution, the truth to-be-found. On the other hand, the poet-architect-adventurer sees before him a landscape inexhaustibly rich in suggestions and materials for making things, for making works of literature or art or technology, for making policies and history itself, or perhaps for making the complex, delicate, existential system called a business.

Thus the puzzle of applied ethnography must involved translations between explanations of truth-seekers and truth-makers. This translation, however, is more possible for some research approaches than others. If we are to assume that research must respond to the world of practice than vice versa, then the "originative" acts of truthmakers would suggest that for the puzzle to be set, social research must be able to "anticipate" and thus lessen the emphasis on prediction that currently holds sway. Seemingly an applied ethnography must be even more a science of becoming than we have thus far achieved, especially

if research hopes to inform politics as well as administration. Shackle (1966: 758) further elaborates the puzzle by focusing on the questions of decision:

> My first proposition is that decision is choice amongst the products of imagination. All we know or can know concerns what is past.... Everything we know about the future is an inference, the end of a reasoning process, whether or not the reasoning is sound. But decision is wholly concerned with the future. Decision is choice of future action aimed at results which we look for in the further future. Thus, decision cannot be the choice of facts. We do not find displayed before us a range of entities which, at one and the same time, are facts already realized and therefore, observable, and are also hypotheses, figments, imaginations of what might come true in some future remote or immediate. The questions for the investigator of decision are: (1) Does the past repeat itself? In what sense, and in what circumstances does it do so? How can we feel whether it will repeat itself? For to know that hte past is going, in known respects, to repeat itself, is know some part of the future in those respects. (2) When the kind of degree of repetition that we can rely on are only sufficient to circumscribe, and not to describe with precision and certainty, those aspects of the consequences of present action which concern us, how can we adapt our policy-decision to this lack of knowledge?

The general form of the puzzle of the policy sciences thus seems to be substantially different than the puzzles of the social sciences. The two general puzzles of the policy sciences seem to be: 1) how much will the future be like the past; and 2) how much is decision like knowledge. LeCompte, however, seemingly implied that more detailed findings in context would be the rudiments of the solution. However, it seems that a more radical departure from normal social science is necessary, if we are to achieve an ethnographic synthesis that can inform practice.

However, it should be evident that explanation for policy purposes will not suffice as an explanation for social science purses, and that practical research does engender some threats .

In applied research the threat to the researcher is that of conversion from intellectual to technician. Merton (1957: 217) succinctly formulates the

problem:

> From all this arises the dilemma facing the intellectual who is actively concerned with furthering social innovation. Not too inaccurately, this may be expressed as a slogan: he who innovates is not heard; he who is heard does not innovate. If the intellectual is to play an effective role in putting his knowledge to work, it is increasingly necessary that he become a part of a bureaucratic power-structure. This, however, often requires him to abdicate his privilege of exploring policy-possibilities which he regards as significant. If, on the other hand, he remains unattached in order to preserve full opportunity of choice, he characteristically has neither the resources to carry through his investigations on an appropriate scale nor any strong likelihood of having his findings accepted by policy-makers as a basis for action.

The dilemma, however, is clearly more involved. The applied research borders on classic alientation, separating thought from action, as he or she serves the interests of the powerful in the "policy space" (Rossi, 1980) which they are allowed. Thus surrender substantive discussion of goals for detailed study of means. As Rossie (1980: 897) warns:

> It should be kept in mind, however, that applied social research is no occupation for would-be philosopher kinds. The applied researcher ordinarily does not get very close to the seats of decision making and policy formation.

The threat to the researcher, unfortunately, is second to the threat to knowledge. Tailloring research to the interests of the powerful will do little to advance knowledge and understanding. At best the result would be a proto-theory having all the trappings of theory but lacking in one fundamental aspect -- perspective. It transforms our understanding of the social meanings and multiperspectival realities (Douglas, 1976) that humans attach to everyday life. Further, it <u>requires</u> that knowledge is useful <u>to the powerful</u>. Knowledge is produced to fit the characteristics of the demand (and the puzzle), including limits on perspective, time and funds and thus approach to be used.

Utilitarian culture is an enemy of applied ethnography (Rist, 1980). The limits placed upon it may mitigate its ability to be able to both apprehend and comprehend the social reality at issue. More importantly, however, utilitarianism engenders a substantive theory that research will not be able to critique. A utilitarian culture implies an appropriate interpretation to events:

> Utilitarian culture also has other consequences of considerable importance for social theory. Most particularly, it entails a shift from the traditional definitions of the object-world in which the moral dimension (the "goodness-badness" dimension...) was comparatively salient, to definitions in which the power dimension (their "strong-weak" dimension...) becomes increasingly salient. Utilitarianism's focus on consequences engenders an increased concern with the sheer potency of objects as a way of achieving desired outcomes, independent of the moral dimension. It is thus not simply that utilitarianism fosters a concern with cognitive judgments as distinct from moral evaluations, but that cognitive judgments themselves come to center on judgments of potency. In this view, to know what is, is to know what is powerful; knowledge is power, when knowledge becomes a knowledge of power (Gouldner, 1970: 84).

Utilitarian culture attempts to redefine the nature of social research and even the meaning of the variables of factors that we identify. Further, it will select, because of the interest in power, deterministic causal chains as the subjects of interest. The threat of utilitarian culture, then, is its reification of rationalistic models that seek to explain struggle, power, and emotion as technical problems in prediction and control, and thus propose administrative solutions over a politica one: it attempts to transform political problems into technical, administrative problems. Rossi (1980: 897) writes:

> Applied social research tends to be conservative, devoted mainly to the examination of policy alternatires that are not radically different from existing social policies. Fine tuning, rather than revolution, is on the political agenda. At best, applied social research is politically congenial, both to those who are liberals and to right of liberals.

The threat of policy-relevant research then lies in the constraints policy puts on the relevant puzzles for investigation. Yet, it would seem if explanation entails a conscious and explicit comparison and translation of ethnographic hypotheses into practice hypotheses that the threat is somewhat lessened. The puzzle maintains both hypotheses and this explicit comparison may be sufficient to guard against the threat of utilitarian culture on ethnographic synthesis. Nevertheless, to the extent that research in education is applied research, the issue will not disappear.

LeCompte's critique of the policy-relevance of the desegregation ethnographies' synthesis then is revealed primarily to be a technical complaint that the synthesis does not entertain her explanation of the failure of desegregation. Her critique is pointed and seemingly accurate. Yet it does not lead us to an adequate account of how the ethnographic synthesis could be policy relevant.

Some Conclusions

I have attempted to account for LeCompte's assessment that the summary attempts of the desegregation ethnographies were failures. LeCompte seemingly did us a great service in defining the issues. She pointed both to the problems of explanation and policy-relevance. However, LeCompte was less able to provide direccion as to the solutions to these failures, which of course was beyond her charge for the critique. Nevertheless, it is instructive to note that her implied solutions, more grounding and detailed description and more theoretical explanation, do little to attain an ethnographic synthesis. What was apparently needed, I have argued, was a theory of social explanation and a comparative understanding of the nature of research and practice.

While I would argue that the summary attempts of the desegregation ethnographies are but a case in point in the problem of ethnographic synthesis, it is important to remember that the failure to attain synthesis does not critique the individual ethnographic analyses or the research program of NIE. Seemingly, both were successful. The five sites individually yielded detailed descriptions of school desegregation in process and NIE gained considerable information to inform their research program. It was when the research teams and NIE thought the studies were sufficiently powerful to warrant synthesis that the problems ensued. If this paper has informed ethnographic sysnthesis as the result of an earlier (but unsuccessful) attempt, then even this aspect of the NIE program has been successful.